Cisidæ with others. Personally, I would much prefer the latter as the permanent form for the word.

Thos. L. Casey

RESEARCH PROBLEMS "ASSIGNED TO" UNIVERSITY PROFESSORS AND THEIR STUDENTS

A paper on North American Forest Research[1] has recently been issued, giving a résumé of the "Investigative Projects in Forestry and Allied Subjects Conducted by National, State and Provincial Governments, Schools of Forestry, Scientific Schools and Private Interests in Canada, Newfoundland and the United States for 1919–20."

More than five hundred projects are enumerated, nearly half of them under investigation by persons in departments of the United States government. Many of the remainder are concerned with the activities of various state agencies and institutions, while a number represent research undertaken by professors and their students in various colleges and universities.

The compilers of this list have very carefully indicated in connection with each project, by whom it is being investigated, nearly always stating that it is "assigned to" some individual or group of persons. For example, under certain universities and colleges, we find numerous projects "assigned to" various members of their faculties and in certain cases secondarily to their students.

I think we may legitimately inquire by whom these problems have been assigned to the persons named. Certainly not by the National Research Council, not by the Society of American Foresters, not by our colleagues, and usually not by any of the governing boards of the universities and colleges.

Such wording, like the repeated use of "control" and "direction," conveys the imputation that men of science do not select and elaborate their own lines of research, and it is very unfortunate that it should appear in such a journal as the *Bulletin* of the National Research Council. Let us hope that the council does not stand sponsor for it, for it does not seem likely that it will aid in attaining the closer cooperation which independent workers hope to see as a result of the operations of the Research Council. It is better to believe that the printer or the proofreader has inserted this stereotyped phrase as it appears quite regularly, and no doubt properly so, in connection with many of the bureaus and governmental agencies. In view of the increasing extension of the bureaucratic spirit into scientific work, perhaps all research must be assigned by some one other than he who performs it, and possibly problems should not be outlined by those who investigate them. Until such comes to pass, however, it seems unjustifiable that research in forestry or in any other subject should be thrust into the lime-light with such carelessly worded captions attached.

C. T. Brues

[1] Compiled by the Committee on American Forest Research, Society of American Foresters, and published as Vol. 1, Pt. 4, of the *Bulletin* of the National Research Council, August, 1920.

THE LAWS OF HYBRIDIZING DISCOVERED BY RICHARD DIENER

The above is the title of a booklet of some sixteen pages, dated (with a rubber stamp) as issued July 1, 1920, and coming, appropriately, from California, the home of plant wizardry. The discoverer states that it has taken thousands of crosses and fifteen years of time to perfect the laws which he is now giving to mankind—for a consideration. Their presentation is a delightful example of simplicity; the reader is not troubled with tiresome descriptions of methods or measures taken to check the results; the pages are not rendered unsightly by arrays of tables, nor is the intellect taxed by incomprehensible statistics, as is so often the case in present-day treatises on this subject. On the contrary the author has not needed all of his sixteen pages for the exposition; besides the title page he is able to spare one for a full-page portrait of himself, five pages are given to photographs of results of his labors, while a double-page diagram sets forth his laws so clearly that

one feels the text might really have been dispensed with entirely.

Nevertheless there are six and one half pages of text, three of which, however, are occupied by a philosophical discussion of "What plant life is," the nature of "Sports," and "Animal life in relation to plant life." The relation of these introductory remarks to the laws that follow is not clear; they nevertheless contain contributions to the subject of evolution which are novel, and their inclusion was presumably considered justified on their own merits. We learn first that plant life is a chemical process for catching the sun's rays and depositing them on the earth in the form of carbon. As with mortal souls, however, the abode of carbon on the earth is but transitory; "some day fire is set to it," whereupon it disappears from the earth as gas and only ashes remain.

Early plants floated in moisture in a sexless state, but they finally succeed in getting roots into the soil, climbed out of the marshes and developed sex, and so rose to the stage of seed production. Until they got their toes into *terra firma* evolution was slow, but that advantage once gained they "developed faster—from grasses to shrubs, from shrubs to bushes and from bushes to trees!" Animals also play an important rôle in the cosmos, for we are told:

> If it were not for the existence of animal life the leaves, bark and general residue of vegetation would, in a period of twenty-five years or thereabouts, cover the ground to such a height that no new vegetation could spring up and plant life would annihilate itself, there being no decay.

About three and one half pages are left for the "laws," which are illustrated by diagrams relating to relative size of flower or fruit or other character of the plants to be crossed. There are three possibilities: (1) The male (pollen) parent may be smaller than the female (ovule) parent, (2) they may be of the same size, or (3) the male parent may be the larger of the two. The first is the "declining way" of breeding, for the offspring from such crosses will be smaller even than the male parent. The second is the "enlarging way," for when the parents are of the same size the offspring will be twice as large as their parents. Not all of them will reach this maximum size, we learn to our disappointment, but on the average only 12 in 100. This may be brought up to 40 per cent., nevertheless, in later generations. Finally, the third way is less important, for under these conditions the offspring are said to exceed the male parent only slightly in size.

Fortunately the benefits of these laws are not limited to plants but may be applied in animal breeding as well, as "exemplified by chickens." Here the process is admittedly complicated by the fact of "the sexes being in different individuals," necessitating a backcross of the progeny with their male parent, but the result is well worth the extra trouble, for "of the offspring from this second fertilization about one third are double the size of the original parents." This may be a desirable economic result so far, but one shudders to think what may happen if the method should be taken up by unthinking persons and pushed to the limits of geometric progression. The author truly says that "few people at the present time realize the immensity of this discovery to mankind." He himself modestly admits that it is "equal to the discovery of electricity, if not greater." And any one may take advantage of it by purchasing the booklet for the sum of five dollars—as indicated by another rubber stamp.

A book of this character would scarcely be deserving of so much attention if it were not for the fact that it is likely to be taken seriously by a great many people. There is just enough of fact in some of the statements to make the conclusions seem plausible to one not familiar with genetic interpretations. For example, it is stated that in attempting to derive new colors, a white flower should be used as the pollen parent. Every geneticist knows that white flowers may carry a great variety of genes for color which can find expression only when a cross is made which brings in an activator for them. Similarly, some of the facts stated in relation to size inheritance may be true in the instances

cited; the mistake is the one practical breeders have so commonly made for generations past of generalizing from a few instances. One often wishes it were as easy to inculcate into students the principles of genetics as it is to gain a wide acceptance of theories that have no scientific basis and calmly disregard any demands for proof.

L. J. C.

SPECIAL ARTICLES

ON THE RELATIONSHIP BETWEEN FREEZING POINT LOWERING, Δ, AND SPECIFIC ELECTRICAL CONDUCTIVITY, K, OF PLANT TISSUE FLUIDS

THE problem of the contribution of non-electrolytes, of undissociated molecules of electrolytes, and of dissociated ions of electrolytes to the depression of the freezing point, Δ, in terms of which osmotic concentration is usually measured, is one of considerable biological importance. We desire to know, for example, whether an observed difference in the osmotic concentration of the tissue fluids of a species growing in two different habitats is due primarily to differences in the quantities of electrolytes absorbed from the medium or to differences in the quantities of organic substances elaborated. The same question naturally arises when one is comparing the osmotic concentration of the tissue fluids of different species in the same habitat.

In the mixed solutions with which the biologist has to deal the problem presents serious difficulties. In certain cases some progress may be made by determining the correlation between the freezing point depression, Δ, and the specific electrical conductivity, K.

As a specific illustration we may take the relationship between osmotic concentration and electrical conductivity in a series of plant species growing in the non-halophytic habitats of the north shore of Long Island.[1]

In a series of 19 species of trees, 36 species of shrubs, and 162 species of herbs both Δ and K are highly variable. The coefficients of variation, *i.e.*, 100 σ/m, where σ is the standard deviation and m the means are:

	Δ	K
Trees	21.46	28.49
Shrubs	18.46	28.03
Trees and shrubs ...	20.20	28.27
Herbs	23.46	25.33

Our problem is to determine whether higher values of K are associated with higher values of Δ, or whether within each of these growth forms[2] these two constants of the solution are essentially independent.

Determining the correlation coefficients by the usual product moment method we have the following measures of relationship between the magnitudes of K and Δ in the various series.

For trees, $N = 19$, $r = +0.127 \pm .152$
For shrubs, $N = 36$, $r = -0.079 \pm .112$
For trees and shrubs, $N = 55$, $r = +0.022 \pm .091$
For herbs, $N = 162$, $r = +0.150 \pm .052$

For ligneous plants the correlations between Δ and K are low and statistically insignificant in comparison with their probable errors. The coefficient for shrubs is actually negative in sign. That for trees and shrubs together is sensibly zero. The coefficient for herbaceous plants is also low but may indicate a slight relationship between the two constants, higher values of Δ being associated with higher values of K and *vice versa*.

These results show that, in the vegetation of the glacial moraines of Long Island at least, there is practically no relationship between the concentration of ionized electro-

[1] Protocols of data and full details are given in a paper in press in the *Journal of Physical Chemistry*.

[2] It is necessary to separate the growth forms, since, as shown in detail elsewhere (Harris, Gortner and Lawrence, *loc. cit.*), the growth forms are highly differentiated with respect to both Δ and K. The actual means are:

	Δ	$K \times 10^6$
Trees	1.292	11,213
Shrubs	1.177	10,770
Trees and shrubs ..	1.217	10,923
Herbs	0.846	14,308